tion, soit à la main, soit à l'aide d'un mouvement d'horlogerie, maintenir l'axe au centre du champ.

Ce procédé présente un avantage marqué sur l'emploi du verre neutre : les quelques inconvénients signalés plus haut, qu'il laisse encore subsister, ne nous paraissent pas de nature à exercer une influence sensible sur les résultats que l'on peut obtenir par la méthode d'extinction : nous avons même pu constater, par l'expérience, que le disque stellaire, malgré le rétrécissement de l'ouverture, n'offre à la vue aucune différence appréciable d'aspect.

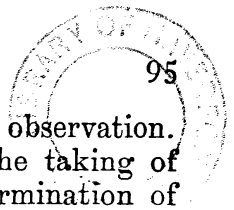
Nous étudions en ce moment une nouvelle disposition qui nous permettra d'atteindre dans des recherches photométriques la précision la plus rigoureuse, et nous espérons pouvoir en donner bientôt la description.

Note on the Employment of Photography in the Transit of Venus of 1882. By E. W. Maunder, Esq.

There is a very general feeling that the results obtained from the photographs taken during the Transit of *Venus* in 1874 are not sufficiently satisfactory to make it worth while to employ photography in the approaching Transit. There is, however, some probability that the circumstances of the Transit of this year may render it possible to attack the problem of the solar parallax in a wholly different manner to that employed in the treatment of the 1874 photographs. And as I have not seen this feature of the 1882 phenomenon adverted to in this connection, I have thought that it might be worth while to draw attention to it, though I cannot think that it has wholly escaped notice.

In measuring the 1874 photographs, the object sought to be attained was the determination of the distance between the centres of the Sun and of *Venus*. This necessitated of course the precise measurement of the position of the limbs of the Sun, and practically of the planet as well. But it was found that the limbs of the Sun were not measurable with anything approaching the required accuracy, and that *Venus* herself was also often strangely distorted. Instead of a hard, sharp, regular line, marking the frontier between light and darkness, the Sun was found to melt away gradually to nothingness, and the difficulty of determining *where* on that delicate shading was the true edge of the Sun, was often aggravated by the irregularities of a "boiling limb." The case was made worse in the English photographs by the need of arriving at the true radius of the Sun in order to find the value of the scale.

Some part of these difficulties were due, I think, to defects which might be readily obviated on a second attempt, for the uncertainty in the determination of the Sun's radius was certainly greater than that usually found in the measurement of the



series now being taken at Greenwich for Sun-spot observation. But it does not seem at present possible to *ensure* the taking of Sun-pictures in which the uncertainty in the determination of the position of the limb shall not be large as compared with the uncertainty in the solar parallax. It is possible, however, that in 1882 the parallax may be inferred without the measurement of the Sun's limb at all.

The Transit of 1874 happened at a time when the solar activity was rapidly declining, and as *Venus* passed across the Sun at a great distance from the centre running almost along the fifty-ninth parallel of North latitude, to which its path was but very slightly inclined, there was no possibility of its encountering or even approaching any solar marking which might serve as a fiducial point, for at that period spots did not wander further from the equator than latitude 15°, and faculæ than latitude 20°, and the manifestations of both orders of phenomena were becoming feeble. In December, 1882, on the contrary, we shall have reached, or at all events be not far from attaining, the time of maximum activity, and *Venus* will travel along a path which crosses the forty-second parallel of South latitude at a small angle. The planet's limb, therefore, from a southern station would be seen to touch the thirty-seventh parallel. It would not be an unprecedented event if there were a spot or spots in a position to be actually occulted by *Venus*, but the probability is very considerable that spots will be found between latitudes 20° and 30°, for this is the favourite *locus* of spots at times of greatest disturbance; so that there is good reason to expect that at some time during the Transit the limb of *Venus* will be within some two or three minutes of arc of a spot. And it is even more likely still that faculæ will be found near the one limb or the other, perhaps near both, extending across the planet's path.

In a lightly-exposed and slowly-developed picture the solar details are seen with great distinctness, and not only spots, but faculæ, and even less conspicuous and defined markings still, often offer easily recognisable points, the positions of which could be undoubtedly measured with the high degree of accuracy required. If then at one or more stations a long-focus photo-heliograph were employed similar to those used by Lord Crawford and the Americans, and by this instrument the distance between several well-marked points on the Sun's surface determined, a scale would be obtained that could be used for the other photo-heliographs, and no necessity would exist for measuring the Sun's limb at all. And it would, I think, be found that *Venus* would show a far more definite and regular outline on pictures taken with a small exposure than in the more fully exposed pictures of the last Transit.

In this manner the two great difficulties attending the reduction of the photographs of 1874 would be overcome: the limb of *Venus* would be more measurable, and that of the Sun left unattempted. There would be the further advantage of the

maximum distance to be measured being only three, four, or at the most five minutes of arc, instead of the whole diameter of the Sun, whilst the relative displacement of *Venus* as seen from two compared stations would generally exceed half a minute.

Then the combination of the pictures would be unrestricted, except that it would be necessary that only photographs which were taken nearly simultaneously should be compared. Thus if soon after ingress *Venus* were found to be near spots, or amongst faculæ giving one or two good points of reference, we might compare pictures taken, say, at Greenwich, Honduras, the Falklands, and Natal; and these might be combined in any pairs, the four photographs thus giving six determinations of parallax. Or near egress, photographs taken at San Francisco, at Auckland, and at Cordoba might be compared, when the three photographs, each taken with a Sun more than 30° high, would give three determinations of parallax, the factor in each case being fully 0·70.

This free combination of photographs would tend to in some degree eliminate the various sources of error. Thus where only three pictures were compared, three positions of *Venus* would be obtained, forming a triangle, the angles of which would be most precisely known. It is clear that no unsuspected cause of distortion, atmospheric, instrumental, or photographic, could so operate as to alter the length of the sides of the triangle—that is, the parallactic displacement of the planet—without betraying its presence by altering the angles as well. I fear the method of contacts provides no such check upon the errors to which it is liable.

If fortune so far favour the observers of the next Transit that *Venus* should occult some spots or well-marked faculæ, then the photographs will be of additional service, not only as themselves affording the means of determining afterwards the times of disappearance and reappearance, and as giving a determination of the parallax comparable in method with contact observations, while free from many of their drawbacks, but by enabling observers to note the times of the phenomena with their telescopes, with the consciousness that the particular marking observed could be afterwards identified.

But if, despite all the probabilities of the case, the Sun disappoint our legitimate expectations, and present a clear, unruffled countenance to us on the sixth of December next, then I venture to think that the following slight alterations might tend to secure an improvement in the results from the measurements of the separation of the centres. First, if collodion be used, the substitution of a wet process for a dry. The American photographs, which seem to have proved decidedly more measurable than our own, were wet plates, and the only good photograph secured by the English photographers was obtained by the same method.* Besides, the wet plates taken at Greenwich on every fine day certainly show limbs better adapted for measurement than do

* *Monthly Notices*, vol. xxxviii. p. 509.

a heavy percentage of the *Venus* plates. Some of the new gelatine processes might give even better results, but I have no personal experience of the behaviour of these plates under the micrometer, and I should not be inclined to expect any very good results from them. Next, a shortened exposure, and if possible a better means of making it than by the drop-slit as at present arranged. Thirdly, some means of securely clamping the instrument. The Dallmeyer photoheliographs are sadly deficient in clamping power. Lastly, a small finder added to each instrument would be a great improvement, and would have prevented the unfortunate failure of the Janssen method at Honolulu.

The most important station for photographic work would be the Falkland Islands; Santiago and Cordoba and other South American posts ranking next, as in all probability photographs will in any case be taken, both in Europe and the United States, supplying the needed corresponding northern stations. A photo-heliograph in Cape Colony and another in New Zealand would be desirable, but less essential. There is one already at work in the Mauritius, where ingress is visible at sunset, but the sun will be very low there. Cuba or Central America would also be good positions, but the post most necessary to be occupied is, as has been already stated, the Falkland Islands, there being but a poor chance of fine weather in the Straits of Magellan.

Blackheath:
1882, *January* 12.

The Relative Motions of the Great Red Spot and Brilliant Equatoreal Spot on Jupiter. By W. F. Denning, Esq.

In the early part of the night of December 24 the sky was shrouded in thick haze, through which *Jupiter* shone very dimly; but at about $9^h\ 35^m$ the atmosphere became much clearer, and I was enabled to observe the markings then visible on the planet. At $9^h\ 43^m$, with power 150 on my 10-inch Reflector, I noted the middle of the red spot and the bright equatoreal spot crossing the central meridian together. I had anticipated from comparative observations of the two objects on the nights of December 20, 22, and 23, that they must occupy the same longitude at this time, and the fact was thus proved by actual observation.

Now, on November 19, 1880, at $9^h\ 23^m$, I observed the same markings in conjunction as they came to transit; and I have watched the motions of these objects, as frequently as opportunity permitted, during the interval of 400 days and 20 minutes which has elapsed between the two observed conjunctions of November 19, 1880, and December 24, 1881.